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Citation for published version:
Henry, J 2012, Why Thomas Harriot was not the English Galileo. in R Fox (ed.), *Thomas Harriot and His World: Mathematics, Exploration, and Natural Philosophy in Early Modern England.* Ashgate Publishing, Aldershot, pp. 113-137.

Published In:
Thomas Harriot and His World

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[Published in: Robert Fox (ed.), *Thomas Harriot: Mathematics, Exploration, and Natural Philosophy in Early Modern England* (Aldershot: Ashgate, 2012), pp. 113-37.]

Introduction

Beginning with Baron von Zach (1754–1832) in the eighteenth century, Harriot has been seen as an English Galileo. Reaffirmed in the late twentieth century by Jean Jacquot and other devotees of Harriot, this claim has most recently been defended by Matthias Schemmel.[1] It is important to note, before going any further, that in many respects this claim is entirely justifiable. The title of English Galileo is based on Harriot's independent discovery of the parabolic trajectory of projectiles; studies of impact and force of percussion or collision which approached closer to the classical solution than Galileo managed; and the independent manufacture and use of telescopes to observe the surface of the moon, and the phenomena of sunspots before Galileo.[2] Like Galileo, he also tried to develop an atomistic theory of matter, although for different reasons—in Galileo's case, atomism was used chiefly as a means of explaining cohesion and the difference between liquid and solid states, while Harriot seems to have regarded atomism as the best system to explain condensation and rarefaction, specific weights, and optical refraction.[3]

Furthermore, Harriot can also be compared with other great figures in the history of science. Take René Descartes, for example. Descartes has a claim to being the independent discoverer in optics of the law of refraction which is now known as Snell's law (after Willebrord Snell)—but Harriot beat both Snell and Descartes to it.[4] It is also arguable that Harriot not Descartes should be credited with the invention of the algebraic technique of shifting all the terms of an equation to the left hand side and making them equal to zero.[5] Evidence that Harriot denied the principle of celestial circularity, and dared to believe that planets might not move in perfect circles, even before the appearance of Kepler's *Astronomia nova* in 1609, has led to an honourable mention as the English Kepler. In what follows, therefore, we will occasionally compare Harriot to Descartes and Kepler, as well as to Galileo.[6]

In spite of the undeniable similarities between the work of Harriot and these most eminent of his contemporaries in the history of science, however, I want to reject suggestions that he was, to stick to my title for now, an English Galileo. It is important to state at the outset, however, that this paper is not concerned to deny, much less refute, all earlier claims that have pointed to the undeniable similarities between the work and intellectual achievements of Galileo and Harriot. This is not, therefore, intended to be a refutation of Dr Schemmel's work, for example. Dr Schemmel has concentrated on Harriot's mathematical work and its similarities to Galileo's mathematical work—the validity and the value of this work is not in dispute in what follows. This paper merely seeks to make a more general historical point, which has important historiographical implications, about the considerable differences between Harriot and Galileo.[7] In part, my aim in exposing these crucial differences is to explain why, for all his astonishing genius, Harriot failed, in the judgement of history, to scale the heights that Galileo did. My denial that Harriot was an English Galileo should not be seen, therefore, as an attempt to belittle this great English thinker, but as an attempt to explain why so often he did not capitalize on his innovations and achievements.

Before I begin give my own reasons for denying that Harriot was an alternative Galileo, it is perhaps worth pointing to another denial based on very different grounds. Stephen Pumfrey has recently pointed to the very real differences in the situations of Galileo and Harriot as a result of the different styles of patronage they received.[8] These differences were so great, Pumfrey argues, that it had a very real bearing on the final achievement of both men, and resulted in Harriot being Harriot, and Galileo, and only Galileo, being Galileo.

Although Galileo began his career as a professor of mathematics at the university of Pisa, and subsequently at Padua, after the publication of his sensational *Siderius Nuncius*, he seized the opportunity to ingratiate himself with Cosimo II, Grand-duke of Tuscany, and remained a leading figure in Cosimo's court until his fall from grace in 1633. His role at court was to advance Cosimo and the Medici in their cultural rivalry with the courts of neighbouring city states, including the Papal court in Rome. Accordingly, Galileo had to be ostentatious, producing work that would make a big noise through the republic of letters; making his own name, and simultaneously enhancing that of his patron.[9]

It was very different in Elizabethan England. Once Harriot left the employ of his first patron, Sir Walter Ralegh, whose demands on Harriot's expertise were almost entirely pragmatic, and became, from about 1595, the pensioner of Henry Percy, Ninth Earl of Northumberland, there was perhaps some expectation that Harriot would enhance the reputation of the Wizard Earl, but it was clearly a far cry from the demands on Galileo as courtier.[10] For one thing, Henry Percy was a courtier himself, serving his monarch, and was not involved in the same kind of rivalry with fellow courtiers as Cosimo was with neighbouring princes. Percy was involved in a rivalry for the monarch's favour, not for his own aggrandizement. Furthermore, it followed from their markedly different positions in their respective political hierarchies, that Cosimo and Percy did not have the same motivation for supporting these two mathematicians. Unlike Cosimo, Percy was genuinely interested in the work of his so-called three magi, Harriot, Walter Warner and Robert Hues. He shared their passion for an understanding of the natural world. Again, this meant that Harriot's work did not need to be played out so publicly as Galileo's. Cosimo had no use for a hermit-like scholar, burning his light under a bushel, but Percy seems to have felt well satisfied by a few days spent with Warner or Harriot performing a fruitless series of alchemical manipulations, or trying to learn how to solve various problems using the latest algebraic techniques.[11] Galileo had to produce work which would have a high impact, sending out shock waves through the republic of letters. Harriot, who already enjoyed the intellectual engagement of his patron, could afford to be 'contented with a private life for the love of learning that I might study freely.'[12]

Another difference between the situations of Galileo and Harriot hinged upon what Pumfrey calls connectivity. It would have been an easy matter for Galileo to find another patron, either in a neighbouring Italian state, or perhaps at the Habsburg court in Prague. This meant that a man like Galileo could maintain some leverage over his patron, ensuring at least some means of maintaining conditions at court that suited him. In England there were few opportunities and a man like Harriot, who was generously retained, had to consider himself lucky. Even John Dee, after all, never found a similar permanent position in England. He was commissioned by Elizabeth for various tasks, but never succeeded in persuading her to make him her court magus. His only recourse was to take up offers of patronage on the Continent—a move which then made him suspect back in England. Harriot did try to find favour with other possible patrons, notably Robert Cecil, Lord Salisbury, and even the young Prince of Wales, Henry, but they died before they could do Harriot any real good. What this meant, then, was that Harriot was never able to break free of the somewhat baleful influence upon him of the reputations of his two successive patrons, Ralegh and Percy. The historical jury is still out, pondering whether Harriot was an atheist or not, but the contemporary jury found him guilty. A substantial portion of his guilt seems to have been acquired by association. If Harriot's reputation for impiety, or for political chicanery, were not of his own making, he could do nothing to dissociate himself from those who were to blame, because of the difficulties of finding patrons in England.[13] In that sense, then, Harriot was in a worse position than Galileo—whose troubles with the Church *were* largely of his own making.[14]

There can be no doubt, then, that differences in the nature of their patronage, stemming either from the socio-political differences in the nature of patronage in early seventeenth-century Tuscany and England, or from the personal differences and demands of their respective patrons, ensured that Harriot and Galileo marched to the beat of different drums, and this very much affected their output of work. I entirely endorse Pumfrey's claim, therefore, that our understanding of the nature of patronage as it affected the careers of both men is extremely important and helps us to see why, in spite of so many might-have-beens, Harriot did not achieve what Galileo did, and did *not* become the English Galileo. Or, rather, perhaps we should say that Harriot's failure to rush his telescopic observations of the moon, or of sunspots, into print meant that Galileo is not now routinely described as the Italian Harriot.

I believe that there is yet more to be said on this matter, however. I believe there is another significant reason why Harriot cannot seriously be regarded as an English Galileo. And it is this aspect of Harriot and his work which I want to focus on for the rest of this essay. I can bring to the fore just what this is by reminding you of a famous aspect of Galileo's negotiations with Belisario Vinta, Cosimo II's Secretary of State, when he first arranged the terms of his appointment to the Ducal Court. 'As to the title of my position,' Galileo wrote,

> I desire that in addition to the title of 'mathematician' His Highness will annex that of 'philosopher'; for I may claim to have studied more years in [natural] philosophy than months in pure mathematics.[15]

And so it was that Galileo became mathematician *and philosopher* to the Grand Duke of Tuscany. Galileo lived up to this title in a way that no contemporary could have foreseen. He was, of course, a leading figure in the transformation of natural philosophy into something much more recognisably like modern science, by amalgamating speculative natural philosophy with mathematical and experimental traditions. Thomas Harriot, however, always remained first and foremost a mathematical practitioner. While Galileo clearly wanted to be seen as a natural philosopher, the evidence suggests that Harriot always remained reluctant to venture into natural philosophy, and therefore, I submit, was unable to participate in its transformation into a new form of philosophy.

So, what I must do now is to explain what I mean by this transformation of natural philosophy, in the course of which it was amalgamated with mathematical and experimental traditions. Only when we have a clear idea of shifting boundaries in the organization of knowledge in Harriot's day can we see where Harriot's work was located. I hope then that I will be able to persuade you finally that Harriot could not have been the English Galileo, because Galileo was a natural philosopher, albeit of a new breed, but Harriot never was.

The separation between mathematics and natural philosophy in Harriot's day

There was a clear distinction still in force in the sixteenth century between natural philosophy, sometimes simply called physics, or *physiologia*, and the mathematical sciences. According to the Aristotelian precepts, natural philosophy was concerned with understanding the 'why' of things, and this required a knowledge of causes, how these things came to be as they are. So, natural philosophy was concerned with causal narratives which not only identified the causes, but showed how the causes operated to bring about the state of affairs in question. Implicit, if not explicit, in this starting point was the assumption that natural philosophy dealt with sensible being, material bodies as they appeared to the senses, and this in turn marked natural philosophy out from mathematics, which considered quantitative being, and metaphysics, which dealt with being in itself, abstracted from any substantial instances of being.[16]

Generally speaking, mathematics was seen as an ineffective way of pursuing natural philosophical enquiry. Mathematics could not provide the required causal accounts, but could only provide what amounted to a special kind of precisely detailed description. Analysis of the motion of the sphere of Mars, for example, could reveal that it must move on a large epicycle around a deferent, and could determine the required speeds of motion on both epicycle and deferent, the amount of eccentricity of the deferent relative to the Earth, and so on. It could not, however, offer any account of why Mars moved this way—of what caused these complicated movements.[17]

Nevertheless, it was recognised that some mathematical sciences came close to the domain of natural philosophy, in so far as they dealt with real physical phenomena. Astronomy was one of these, as was music (the science of mathematical ratios, which clearly corresponded in some way to the physical harmonies heard in music), and optics, the geometrical study of the behaviour of light rays. These were designated in the Aristotelian tradition as *scientiae mediae*, or mixed sciences, because they used the principles of mathematics to understand what was taking place, but were part of physics because they dealt with sensible things. The mixed sciences were said to be subalternated to mathematics on the one hand, and to physics on the other, and there were endless scholastic debates as to which of the parent sciences dominated.[18]

Now, by the second half of the sixteenth century change was already taking place, and the mixed sciences were beginning to play a greater role in natural philosophy. In particular, a new *scientia media*, mechanics, was beginning to be included in discussions of motion in natural philosophy. Mechanics had previously been regarded as an art, rather than one of the mathematical sciences, because its purpose was entirely practical. But the Renaissance re-discovery of the *Mechancial Problems*, attributed to Aristotle (though admittedly never uncontroversially accepted as genuine), made the learned aware of the theoretical credentials of mechanics, and it was accordingly placed alongside the other mixed mathematical sciences. Although mechanics dealt chiefly with processes taking place in artificial set-ups, such as machines, and should therefore have been excluded from natural philosophy, which was supposed to deal with unforced, perfectly natural processes, it was occasionally introduced alongside discussions of projectile motion, which Aristotle had included in the *Physics*.[19]

The increasing attention paid to the mathematical sciences in natural philosophy was due in no small measure to the efforts of a very wide range of different mathematicians to increase the intellectual status of their subject. There were mathematical humanists, recovering ancient mathematics, university mathematics teachers, and a wide range of mathematical practitioners, from elite architects and military engineers to more humble teachers and artisans, all of whom were trying to make a living from mathematics and, in many cases, to exploit the interconnected possibilities for patronage which existed on continental Europe, if not in England.[20] Nevertheless, there was a great deal of inertia in scholastic natural philosophy, and in the neighbouring disciplines, so there was still a great deal of separation between natural philosophers and mathematical practitioners.[21]

The classic illustration of this, of course, is the preface added to Copernicus's *De revolutionibus orbium coelestium* by the Lutheran minister who had been delegated to supervise it through the press, Andreas Osiander. Judging from the opening remark, Osiander had heard of objections to Copernicus's approach to astronomy even before it was published, and sought to forestall them.[22] Accordingly, and without asking for the author's permission, he interpolated a page, before Copernicus's own preface, which bore only Osiander's unsigned preface:

> There have already been widespread reports about the novel hypotheses of this work, which declares that the earth moves whereas the sun is at rest in the center of the universe. Hence certain scholars, I have no doubt, are deeply offended and believe that the liberal arts, which were established long ago on a sound basis, should not be thrown into confusion. But if these men are willing to examine the matter closely, they will find that the author of this work has done nothing blameworthy. For it is the duty of an astronomer to compose the history of the celestial motions through careful and expert study. Then he must conceive and devise the causes of these motions or hypotheses about them. Since he cannot in any way attain to the true causes, he will adopt whatever suppositions enable the motions to be computed correctly from the principles of geometry for the future as well as for the past. The present author has performed both these duties excellently… For this art, it is quite clear, is completely and absolutely ignorant of the causes of the apparent nonuniform motions. And if any causes are devised by the

> imagination, as indeed very many are, they are not put forward to convince anyone that they are true, but merely to provide a reliable basis for computation…
>
> … So far as hypotheses are concerned, let no one expect anything certain from astronomy, which cannot furnish it, lest he accepts as the truth ideas conceived for another purpose, and depart from this study a greater fool than when he entered it. Farewell.[23]

As Robert S. Westman has pointed out, it is not the nature of the physical world which is in danger of being thrown into confusion by Copernicus's heliocentric astronomy, but the liberal arts, to which astronomy has long since been allocated. To reassure his readers Osiander immediately reaffirms that the author is not saying anything about true causes—that is to say, he is not making any natural philosophical claims—but is merely considering hypothetical, we might say simply 'mathematical', constructs which enable us to correctly calculate planetary positions. Copernicus himself undoubtedly wanted to assert a new relationship between mathematics and natural philosophy, but Osiander has undercut his intentions by reaffirming the traditional boundary between natural philosophy and the subalternate mixed science of astronomy. 'The upshot of Osiander's skilfully argued *Letter* is striking', Westman wrote, 'in believing that he has demonstrated the astronomer's *inability* to draw conclusions in natural philosophy, he denies him the *right* to do so.'[24]

The fact that the gulf between natural philosophy and mathematics remained wide even for the succeeding generation of Copernican astronomers can be seen in the fact that the prime movers in establishing the truth of the Copernican astronomy (prime movers at least in the judgement of history) all turn to causal explanations as to how the

Earth moves, and do not assume that the mathematics can speak for itself. In this they differed from Copernicus himself, who famously wrote that mathematics is written for mathematicians.[25] Effectively Copernicus simply hoped that what might be called the mathematical aesthetics of his system would carry the day. He hoped that the fact that his system provided a unique and fixed order of the planets which is entirely compatible with the orbital periods of the planets (unlike the Ptolemaic system, in which the order of the planets was only conventional) would persuade his readers of the physical truth of his system.[26] But such hopes were only realistic, and Copernicus knew it, if his readers were mathematicians—only they could see the point. Kepler was not content to speak only to mathematicians, however, and so the full title of his *Astronomia nova* announced that it was an astronomy based on causes (*astronomia aitiologetos*), or a celestial physics (*physica coelestis*).[27] Whether any non-mathematicians were tempted to take a look at Kepler's book remains doubtful, but Galileo moved almost entirely away from mathematics in his attempts to establish the truth of Copernican theory. He presented the observations he made with the telescope as powerful circumstantial evidence in favour of the Copernican theory, and in his *Dialogue on the Two Chief World Systems* he developed a whole new natural philosophical theory of motion to show how the Earth *could be* in perpetual circular motion, and presented his explanation of the tides to argue that it *must be* in motion.[28]

This brings us, by way of contrast, to Harriot. When Harriot looked at the moon through the telescope he had made, he drew what he saw—just as Galileo was to do some months later. Unlike Galileo, however, he did not set down in writing what he made of this; he did not discuss what general conclusions might be drawn from what he saw, much less tell us what conclusions, if any, he drew from his conclusions. Similarly, he meticulously recorded his observations of sunspots, but he made no pronouncements upon them. His records indicate that he thought of them as spots on the surface of the Sun. He did not make the same mistake as the Jesuit astronomer, Christoph Scheiner, who assumed they must indicate a planet (or planets) circling the Sun inside the orbit of Mercury. Evidently Harriot could tell by the way the spots clustered, their sometimes ragged appearance, and the way they appeared and disappeared, that they were not planets.[29] But whether he ever gave any thought to the fact that these spots undermined the Aristotelian claim that the heavens are perfect and unchanging, we simply do not know. Not only did Harriot *not* write an English *Siderius nuncius*, or a *Letter on Sunspots*, as far as we can tell he didn't even make any private notes about these things to accompany his records of his observations. If he thought anything, he either kept those thoughts to himself, or was content merely to discuss them with his immediate companions.[30]

It is almost as though Harriot had his own Osiander inside his head, a voice of conscience which told him that, as an astronomer he did not have the *ability* to draw conclusions in natural philosophy, and therefore he had no *right* to try to do so.[31]

It may be useful to consider another illustration, which shows on the one hand the continuing gulf between mathematical and natural philosophical work, and on the other the kind of innovatory work Harriot might have done, but did not. This illustration is based on the discovery of magnetic dip or declination, discovered by the retired mariner, turned compass-maker, Robert Norman, and published by him in 1581 in his *Newe Attractive*. Norman seems to have been aware of the phenomenon of declination for a while, but only paid attention to it when it caused him to ruin one of his compass needles. He had noted that a perfectly balanced iron needle, would no longer remain horizontal (as though balanced) on its pivot, after it had been magnetised by rubbing with a lodestone. The needle would now dip down towards the north, as though it were heavier at that end. Routinely, he would shave off some iron from the north pole of the magnetised needle to restore it to the horizontal. He was moved to investigate this further when, on one occasion, he took too much weight off the end of a particularly large commissioned needle and so, 'stroken into some choler', had to start again with another piece of iron.

Norman took advice on how to proceed in his investigations from 'certaine learned and expert men':

> I applied my self to seeke further into this effect, and makyng certaine learned and expert men, my freendes, acquainted in this matter, they advised me to frame some Instrument, to make some exacte triall, how much the Needle touched with the Stone would Decline, or what greatest Angle it would make with the plane of the Horizon. Whereupon I made diligent proofes, the maner whereof is shewed in the Chapter followyng.[32]

Now, it seems to me that these expert friends must have been mathematical practitioners, rather than natural philosophers. I base this judgment not simply on the surely undeniable fact that Norman was much more likely to know mathematical practitioners of various stamps than he was to know any natural philosophers. I also base it on the nature of the trials he was advised to make. The emphasis is all upon measuring aspects of the needle's behaviour. Norman is not advised to make exploratory experiments, but 'exacte triall[s]'. He is advised to try to determine *by how much* the needle declines, not why it does so. When Norman followed this advice he was able to come up with a new navigational instrument, the dip circle, which would reveal a ship's latitude even under cloudy skies, when no heavenly bodies were visible. What he did not come up with was a natural philosophical explanation, in causal terms, of why this phenomenon took place.[33]

Just this kind of natural philosophical explanation for this phenomenon was provided in 1600 by William Gilbert in his famous work of experimental physics, *De magnete*. We don't know when Gilbert became aware of Norman's work but at some point he seized upon it and made it the starting point of a new system of natural philosophy. Gilbert saw that the dip of the needle was due to the fact that the magnetic pole of the needle was pointing directly to the magnetic pole of the Earth, not, as was previously held, to the pole star in the heavens. Pointing directly to the magnetic pole, the needle pointed down below the horizon, through the earth itself. He surmised that at the magnetic pole the needle would point straight down, vertically into the Earth, at the equator, the needle would hang horizontally on its pivot. He was then able to demonstrate that this is precisely what happened when a small magnetic needle was suspended above different parts of the surface of a spherical magnet (a *terrella*, as Gilbert called them). It followed from this that the Earth itself was a giant spherical magnet. From here, Gilbert was able to go on to use the magnetic nature of the Earth to argue for its perpetual circular motions, as demanded by Copernican astronomy.[34]

It seems to me that Norman and Gilbert were two completely different kinds of thinker. Norman was concerned only to know how to put to pragmatic use the phenomenon that he accidentally discovered. His idea of investigating the phenomenon of magnetic declination seemed to consist merely in confirming that it was real and then taking stock of its precise behaviour in order to see how it might be put to use—this enterprise was ultimately embodied in the fact that he invented a new instrument to measure the phenomenon. Gilbert, by contrast, seized on magnetic dip precisely because he saw how it could be used to support Copernican astronomy, by pointing the way towards a *causal* account of how the Earth could maintain itself in perpetual motion. Gilbert's new natural philosophy was innovatory in its content, but it was entirely traditional in its concern to offer causal explanations.[35]

Comparisons are odious, but if we had to compare Harriot with one of these two magnetic experimenters, it seems to me that we would have to count him as closer to Norman than to Gilbert. Harriot's experiments, in whatever area, all seem to have been, like Norman's, in the nature of 'exacte triall[s]', to take measurements, and to carefully record changes. There is no evidence that he ever did an experiment to test, or to demonstrate, a putative theoretical explanation of the nature of the world.

Thomas Harriot: mathematician but no natural philosopher

I believe it is Harriot's refusal to enter the natural philosophy stakes which lies behind the frustration and disappointment which scholars of Harriot often feel the need to express. Whether it be John Shirley commenting on his alchemy ('nowhere does he indicate why he is doing what he is doing, and nowhere does he give any conclusion as to what he has found or failed to find'), John North commenting on his observations on sunspots ('He left no explanations of the phenomena he so painstakingly recorded, and for this reason he simply cnnot be compared in historical terms with Galileo…'), or some other student of Harriot commenting on other aspects of his work ('Harriot himself left no ordered and coherent body of philosophical speculation'), there is no shortage of expressions of frustration and exasperation with Harriot.[36]

As Jim Bennett has pointed out, however, the fault is ours, not Harriot's. Harriot could not have foreseen our historiographical preoccupations, and may not have wished to pander to them if he had.[37] And yet, there has been a strong tendency among Harriot scholars to assume that if he wasn't the English Galileo he should have been! Jean

Jacquot, for example, seemed to want to imply that Harriot did try to reform natural philosophy (like Galileo) but that evidence of his efforts has been lost:

> Considering the scope and originality of his research, one may wonder to what extent he felt the need of a new philosophy to replace the old, and to what extent he was able to shape its elements into a coherent pattern. Unfortunately the evidence to be found in his writings is fragmentary.[38]

This attitude is often held to have been summed up by his friend Sir William Lower:

> Do you not here startle, to see every day some of your inventions taken from you; for I remember long since you told me as much, that the motions of the lanets were not perfect circles. So you taught me the curious way to observe weight in Water, and within a while after Ghetaldi comes out with it, in print. a little before Vieta prevented you of the Gharland for the great Invention of Algebra. al these were your dues and manie others that I could mention; and yet too great reservednesse hath robd you of these glories… Onlie let this remember you, that it is possible by too much procrastination to be prevented in the honour of some of your rarest inventions and speculations. Let your Countrie and friends injoye the comforts they would have in the true and great honour you would purchase your selfe by publishing some of your choise works.[39]

This was written in February 1610 and so it is all too easy to imagine Lower being startled once again soon after when Galileo's *Siderius Nuncius* hit the bookstands, and again when the *Letters on Sunspots* appeared. Accordingly, there has been a tendency to read this passage as an implicit foreshadowing of the claim that Harriot could have been the English Galileo. But this is to read too much into Lower's words. He was almost certainly only concerned that Harriot did not establish his priority, and so a lasting fame (for himself and for his country). There is nothing here to suggest that

Lower had already seen in Harriot's work intimations of a new natural philosophy. Furthermore, there is nothing in subsequent correspondence to suggest that he saw this subsequently.

This is instructive. After all, Lower had actually been present at Syon House, in December 1610, observing side by side with Harriot, when Harriot discovered sunspots. When reporting this in his biography of Harriot, John W. Shirley could not refrain from imaginatively reconstructing their excitement:

> To see actual imperfections in a celestial body ostensibly composed of the perfect quintessence of extra-terrestrial bodies was an almost unbelievable experience… [T]he Syon Philosophers shared the excitement of a discovery which, of itself, spelled the doom of the doctrines of the perfection of the quintessence, or circular motion, cycles, and epicycles; and by showing the imperfections of a heavenly body showed the way for acceptance of more material explanations of the operation of the universe.[40]

It is very easy to see why Professor Shirley wrote this way. To those of us who have ever undertaken a formal course in the history of science, the really astonishing thing about Harriot's discovery of sunspots is that it did not elicit the kind of response that Shirley describes—if what we were told in class is true, then surely Harriot had to be excited at seeing the doom of Aristotelian natural philosophy. Once again we are frustrated to find Harriot merely recording his observations of sunspots without making any significant comment about their significance. What are we to make of this? Is it merely the result of Harriot's 'internal Osiander', telling him that, as an astronomer he has no ability as a natural philosopher, and therefore no right to speculate like one?

Clearly, Shirley was wrong to say that the discovery of sunspots 'of itself' showed the way to a new theory of the cosmos. The discovery evidently required a

Galileo, a mathematician *and* philosopher, to show contemporaries what it implied. But Harriot's failure to make of sunspots what Galileo did still seems genuinely surprising. But once again, perhaps the fault is ours, not Harriot's.

It is possible, after all, to understand why Harriot might not have been as excited as the standard historiographies lead us to suppose that he should have been. We simply have to remind ourselves that it was way back in 1572, shortly after Harriot's birth, that Tycho Brahe had famously seen a new star burning so brightly in the sky that it could be seen even in daylight.[41] Having established by its lack of parallax that this was indeed a new star, and not simply an atmospheric effect, Tycho went on to scrutinise comets in the same way and established that they too gave the lie to the Aristotelian belief that the heavens were unchanging.[42] Even Jesuit astronomers at the Collegio Romano, including Christopher Clavius, conceded that there must be similarities between superlunary and sublunary bodies on the grounds that they do undergo change.[43] Harriot the astronomer grew up, therefore, in a world where the heavens were not perfect and unchanging, and must have learned simply to dismiss Aristotelian doctrine on this matter as an irrelevance. We also know that he was one of the very first true Copernicans, who accepted the physical truth of the motion of the Earth. We can add to this the fact that Harriot probably knew of William Gilbert's map of the moon, which Gilbert included in his unpublished *De mundo*.[44] It is not clear whether Gilbert used a telescope to make his moon map, but when Harriot subsequently came to make his own map with the help of a telescope, he would not have been surprised to see the moon looking like an earthly body, rather than as a glowing disc, or sphere, of unchanging quintessence. (Indeed, he surely would have been surprised if he'd seen anything but an earthly-looking moon).[45] In view of all this, it seems reasonable to suppose that when Harriot saw sunspots he saw them merely as further evidence for the already well-established changeability of the heavens. Presumably, it simply never occurred to him that anything more needed to be said. After all, even Galileo did not publish on the sunspots he had observed until after the Jesuit astronomer, Christoph Scheiner, had tried to save the unchanging perfection of the sun by claiming the spots were really satellites orbiting the sun.[46] In short, Harriot responded to sunspots not as we might have expected a natural philosopher to do, but as an astronomer and mathematician.

It seems to me, furthermore, that we must take the same line with all the many different aspects of Harriot's work. He was always thinking as a mathematician: seeking to make measurements,, to establish certainties and proofs, and to find pragmatic ends. He was much less concerned, if at all, with providing the kind of explanations in terms of causal narratives that were demanded by natural philosophers. However it may look to us with hindsight distorted by our preoccupations, there was nothing in any of his studies that inevitably had to lead him into making a major revision of traditional natural philosophy. And that being so, we cannot take him to task for failing to turn himself from a brilliantly gifted mathematical practitioner, into an innovatory natural philosopher.

Just because we know that Descartes used his mathematical analysis of the rainbow as an example of the success of his philosophy in his *Dioptrics*, which he appended to his *Discourse on the Method* of 1637, it would be wrong to suppose that Harriot's analysis of the rainbow should have led him to write an English *Le monde*. The link between Descartes's geometrical optics and his system of mechanical philosophy was not direct, in the sense that one did not lead straight to the other. It was only once Descartes had his insight into how a mechanical system of philosophy might work that he was able to provide a new causal explanation of light within the terms of that system. He was then required to show that his new account of the nature of light was compatible with geometrical optics, and in that regard he was able to use his analysis of the rainbow to brilliant effect.[47]

Similarly, a study of colliding particles and their reactions does not have to lead to a fully-fledged system of natural philosophy. Again, we know that it did in the case of Descartes, but we cannot infer from that that any such study must also lead to a mechanical philosophy. It did not, after all, in the hands of Galileo, who originally wanted to include a study of impacts and forces of percussion in his *Discorsi… intorno a due nuove scienze*, although it is possible that it might have done had he been able to complete it.[48] It did not do so in the case of Isaac Beeckman, even though he was specifically trying to develop a new system of physico-mathematical philosophy, and even though he came close.[49] If mathematically-minded natural philosophers, like Galileo and Beeckman, could not manage it, there were plenty of less ambitious mathematical practitioners who were content to stay within the confines of mechanics. Harriot seems to have been one of these. As Jon Pepper commented on Harriot's *De reflexione corporum rotundorum*:

> This is in keeping with Harriot's other contributions to science, in which, in my view, the general position which comes through is that of a continual searching for the mathematicising of the physical world in whatever aspect, with little speculation as such being expressed.[50]

It seems to me that the speculation is crucially important, particularly if we mean speculation about explanations and causes. The only evidence we have that Harriot ever did speculate about causes derives almost entirely from his atomism. Even here, however, we have to rely largely upon a second-hand source, Nathaniel Torporley's critique of Harriot's atomism.[51] According to Torporley, Harriot used atomistic precepts to explain three major phenomena: condensation and rarefaction, the refraction of light, and specific gravities. There is no reason to doubt Torporley's account; his claim about the refraction of light is even confirmed by Harriot himself, though only briefly, in a letter to Kepler.[52] So, on the face of it, this seems to provide us with clear evidence that Harriot was willing to play the natural philosopher sometimes.

We are still left in the dark, however, about the extent of Harriot's willingness to play this role. Torporley's *Synopsis of the Controversie of Atoms*, corresponds to no known document written by Harriot himself. It is perfectly possible, therefore, that it represents Torporley's written account of his response to Harriot in what was nothing more than a debate between the two friends in front of other friends. *The* controversy mentioned in Torporley's title is certainly compatible with a debate carried out on a single occasion.[53] What's more, I find it hard to imagine what Harriot could have said in response to what Torporley calls his 'squadrons' in the battle against Harriot. We need not pursue these arguments here, but suffice it to say that they raised long-standing objections to atomism which would not be overcome until Newton introduced attractive and repulsive forces operating between the atoms.[54] If 'the controversie of atoms' was a specific debate held between Harriot and Torporley, it seems likely that Torporley would have won. If, on the other hand, 'the controversie' represented a long standing difference of opinion between the two men, it is still hard to believe that Harriot could have had much confidence in his own position in the face of Torporley's squadrons.

Furthermore, if we add to this the evidence provided by Harriot's papers discussing 'infinites' (in which we must include what would later be called infinitesimals), the papers entitled *De infinitis*, it seems hard to imagine that Harriot could have developed confidence in natural philosophising by drawing upon atomism. Again, we cannot review all the arguments here, but suffice it to say that Harriot was led by his mathematical approach to assume that atoms are indivisible by virtue of being indivisibly small, that is to say, geometrical points. As Aristotle had long-since pointed out, however, that kind of mathematical atom cannot be used to compose physical entities. If physical bodies are to be composed of atoms, the atoms must have dimensions, and so their indivisibility must be defined in ways which are incompatible with geometrical demands for infinite divisibility.[55] The evidence suggests, then, that Harriot might have tried his hand as a natural philosopher using atomism as his explanatory system, but evidence of internal difficulties, and the lack of any signs of development of a fully-worked out system of atomic philosophy, strongly suggest that Harriot found such speculation a frustrating experience—one from which he was perhaps glad to retreat while committing himself instead to the certainties of mathematics.

It might be said that although my arguments against Harriot turning his geometrical optics into a system of natural philosophy, or his theory of impacts, or his atomism into a system of natural philosophy, work individually and in isolation from everything else he did; when taken together, these things surely point to a thinker engaged in developing an innovatory system of natural philosophy. Such an argument seems to amount to claiming that Harriot was the English Descartes. The argument would seem to run like this: we know that geometrical optics, the mechanics of colliding bodies, and a matter theory that was closely modelled on atomism, all contributed to Descartes's mechanical philosophy, and since we also know that Harriot was involved in these same three projects, he was clearly also involved (before Descartes!) in trying to develop a new system of mechanical philosophy.[56] The hidden premise in such an argument is that the mechanical philosophy follows implicitly from the mere combination of geometrical optics, the mechanics of colliding bodies, and corpuscular matter theory. But this is simply to denigrate Descartes's achievement. This is not the place to try to reconstruct how Descartes arrived at his system of mechanical philosophy, but it certainly did not simply become apparent to him as a result of his pursuing these separate strands of thought in parallel. The mechanical philosophy was forged out of these separate strands but in a way that revealed that a unique intellect of great genius was at work.[57] To cite just one example, Descartes's corpuscular matter theory enabled him to adopt aspects of atomism which were highly useful in physical explanation, but by denying that the invisibly small particles of matter as he conceived them were indivisible he avoided the insurmountable pitfalls that Torporley's squadrons raised against Harriot's atomism. Harriot was very definitely not the English Descartes.[58]

I see Harriot, therefore, as one of the contributors to the mathematisation of the world picture, and to the introduction of experimental techniques, and of mathematical instruments, which went hand-in-hand with that mathematising process; all of which has been brilliantly described by Jim Bennett and others in recent years.[59] The rise of the mathematical practitioner, intellectually and socially, is undeniably of crucial importance for understanding the Scientific Revolution, and in this I entirely agree with Jim Bennett, but it should not be forgotten that, as Bennett himself points out, crucial stages in the development of the mechanical philosophy were 'represented by the powerful formulations of individual thinkers'.[60] Those individual thinkers needed to be natural philosophers as well as mathematicians. Where Dr Bennett, quite justifiably, wants to emphasise the role of what he calls the mechanics' philosophy, I want to suggest that to progress from the mechanics' philosophy to the mechanical philosophy, the mechanics needed middle men, men who were not just mathematicians, but who were, like Galileo, and like Descartes, mathematicians *and* philosophers. These were the men who recognised the importance of causal explanation as it was emphasised in the Aristotelian tradition, and who sought to add physical explanations to otherwise purely mathematical accounts.[61]

It seems to me that only a small handful of thinkers succeeded in combining these two approaches—chief among whom were Galileo, Kepler, Descartes, but there were others. We must exclude Harriot from that list, however, on the grounds that his work did not provide the putative causal explanations required for it to count as natural philosophy. There is no need to be ashamed on Harriot's behalf, he was working after all at a time when the separation between natural philosophy and the mixed mathematical sciences was still wide. Even in 1638, when Louis Elzevir published Galileo's *Discourses* he was using a title chosen by himself. We do not know what title Galileo gave to his last great work but we do know that he regretted Elzevir's choice of 'a low and common title for the noble and dignified one carried upon the title-page'. In publishing it under the title, *Discourses and Mathematical Demonstrations concerning Two New Sciences pertaining to Mechanics and Local Motions*, it would seem that Elzevir was playing a similar role to Osiander when he added his apologetic preface to Copernicus's *De revolutionibus*, warning Galileo's readers that this was a book on mathematics. It seems reasonable to assume that Galileo's more 'noble and dignified' title gave the impression it was a work of natural philosophy. Clearly, Elzevir did not see it that way, and felt that his readers would not.[62]

It is also worth remembering that even as late as 1687 the title of Isaac Newton's great book struck contemporaries as puzzling and unfamiliar. To speak of the *Mathematical Principles of Natural Philosophy*, was to bring together two intellectual approaches that still seemed very different and widely separated from one another, as far as most educated readers were concerned. What's more, even Leibniz (the German Newton?) objected to what he saw as a lacuna in Newton's natural philosophy, when

Newton failed to provide a properly *causal* account of the operation of gravity. As is well known, Newton did not respond by providing one, but by invoking the certainty of the mathematical approach: ‘it is enough that gravity really exists and acts according to the laws that we have set forth’.[63] On this occasion, therefore, Newton exploited the lingering division between mathematics and natural philosophy to defend his nescient position on the nature of gravity. This is the kind of response, *mutatis mutandis*, that Copernicus might have given in 1543 if someone had pointed out that he did not explain how the Earth moves.[64]

Given that the separation between mathematics and natural philosophy was still so wide, it is hardly surprising that a consummate mathematician like Harriot (surely the superior of Galileo in this regard) should decide to stick to his practice, as a cobbler sticks to his last. After all, Harriot could not have known that the future lay with an as yet undreamed of mechanical philosophy, which would be the triumphant result of a combination of the mathematical approach with a natural philosophy that still offered a causal narrative about the way the world worked. It seems much more likely that Harriot would still have seen mathematics and natural philosophy as separate and distinct enterprises. This being so, it is highly likely that he would have believed that the future belonged to mathematical practitioners, and that natural philosophy could only lead to dead ends.[65]

Accordingly, I do not think that we have to suppose that Harriot carried around with him an ‘internal Osiander’, a voice of conscience telling him that as a mathematician he was unsuited for philosophising.[66] I believe that even the little that we do know about Harriot’s atomistic speculations is enough to indicate that he was willing to try his hand at natural philosophy, if he thought it would help. The fact that he did not pursue atomism, but evidently turned back instead to the certainties of the mathematical approach is not a sign of failure, or of diffidence, but of an uncompromising perfectionism. Harriot compares very favourably here with Galileo and Descartes, both of whom seem to have been astonishingly blind to the obvious failings of their philosophical speculations.

Although Galileo's theory of the tides can be said to show a powerful intuition about the role of oscillatory systems in tidal phenomena, that intuition went far beyond the bounds of his astronomy, his physics, his experimental method, and his mathematics.[67] Consequently, the tidal theory, upon which he pinned so much, stood out for his contemporaries as an embarrassing absurdity in his physics. Moreover, the fact that his attempt to explain the perpetual motion of the Earth and the other planets depended upon the assumption that the planets move in perfect circles with uniform circular motions, seems wilfully dismissive of the demands of astronomy. The whole history of astronomy since the Ancient Greeks had been an attempt to reconcile the belief in uniform circular motions with the all too obvious observations which unavoidably suggest that the planets do not move uniformly, nor homocentrically. Galileo's argument for the motion of the Earth in terms of a perpetual uniform motion on a flat frictionless plane that, unlike a sloping plane, affords no reason for the Earth to accelerate or decelerate, is ingenious to be sure. But once the reader realises that the 'flat' plane which neither approaches nor recedes from the Sun is in fact a sphere around the Sun, and the Earth's unceasing motion is therefore proved in terms of its uniform circular motion around the Sun, incredulity sets in. It is impossible to read the relevant parts of the *Dialogue on the Two Chief World Systems* without wondering whether Galileo is really serious in developing an argument which does not simply fail to fulfil the Platonic injunction to 'save the phenomena' revealed by astronomy, but egregiously ignores them![68] Similarly, the Cartesian system, for all its breath-taking ingenuity, was shot through with insurmountable difficulties. Its insistence that there could be no new motions in the world, for example, only the transfer of motion from one part of the system of the world to another. And the concept of vortexes, as Newton later spelled out, was 'beset with many difficulties'.[69]

We now know, of course, that the failures of Galileo and Descartes were magnificent failures, providing important stages on the way to our modern understanding of the physical world. But their immediate contemporaries could not have known this, much less a thinker like Harriot, who did not live to see the direction that Galileo and Descartes took in their major publications. If Harriot had tried to be a pioneer in combining mathematical approaches with a causal natural philosophy, it is hard to believe that he could have done any better than Descartes. It is likely that his own putative system of natural philosophy, whatever it might have been, would also have proved inadequate (as for example that of his fellow 'magus', Walter Warner did).[70] It seems clear, however, that Harriot never did try to develop a new philosophy—he was perhaps too clever, or too self-critical, to indulge in self-deceiving philosophising. Throughout his career he concentrated instead on 'exacte trialls' and other more restricted, but more certain, mathematical approaches, perhaps hoping that new understanding would emerge from the certainties of mathematics.

If he did hold out such hopes for mathematics, rather than for philosophical speculation, he could hardly be said to be deluded. Developments in mathematics during the first two decades of the seventeenth century were arguably more exciting, and potentially fruitful, than developments in natural philosophy. Furthermore, it was at just this time, let us not forget, that Harriot's great contemporary, Francis Bacon, was also discouraging speculation in philosophy, and advocating instead a careful gathering of accurate and reliable information about natural phenomena. Harriot's eschewing of philosophical speculation, so regretted by his modern commentators, was in his day by no means a methodologically unjustifiable position.[71]

In conclusion, I want to say that, in spite of the epitaph on Harriot's monument, where he was described as excelling in mathematics, natural philosophy and theology, he was no more a natural philosopher than he was a theologian, and for that reason he should not be seen as the English Galileo.[72] Furthermore, I believe that he concentrated almost exclusively on mathematics as a matter of personal choice and preference. Although it might be said that in the early part of his career, when he was retained by Walter Ralegh, he had no choice but to pursue the entirely pragmatic ends of his patron, this was not the case during the years that he was the pensioner of Henry Percy. As Stephen Pumfrey has pointed out, 'Northumberland was unique in patronizing natural philosophy as well as utility', and we know that Walter Warner must have spent much of his time developing the new system of natural philosophy which can be seen in his manuscript remains.[73] Harriot could have pursued natural philosophy if he had wanted to, but judging from his own manuscript remains he never did.[74] It is impossible to know whether Harriot rejected the natural philosophical approach out of diffidence, brought on by his own 'internal Osiander', telling him that as a mathematician he was unsuited for philosophy; or as a result of a perfectionism which he felt he could achieve through mathematics, but not through the much less certain speculations of physics. Either way, Harriot was not the English Galileo.

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[1] Zach, an Austrian mathematician, re-discovered Harriot's papers during a visit to Petworth House in 1784. For a full account of his attempts to establish Harriot as the

English Galileo see John W. Shirley, *Thomas Harriot. A biography* (Oxford, 1983), pp. 13–23. See also, Jean Jacquot, 'Harriot, Hill, Warner and the new philosophy', in John W. Shirley (ed.), *Harriot, Renaissance scientist* (Oxford, 1974), pp.107–28, at p. 107. Matthias Schemmel, 'Thomas Harriot as an English Galileo: the force of shared knowledge in early modern mechanics', *Bulletin of the Society for Renaissance Studies*, 21 (2003), 1–9; expanded in 'The English Galileo: Thomas Harriot and the force of shared knowledge in early modern mechanics', *Physics in perspective*, 8 (2006), 360–80. I have been unable to consult idem, *The English Galileo: Thomas Harriot's work on motion as an example of preclassical mechanics* (Dordrecht, 2008).

[2] Literature on Galileo is vast, but for an account of his work, see Stillman Drake, *Galileo at work: his scientific biography* (Chicago, 1978). For discussions of the similarities between Harriot and Galileo see Shirley, *Harriot. A biography*; the works by Schemmel cited in the previous note; and John D. North, 'Thomas Harriot and the first telescopic observations of sunspots', in Shirley (ed.), *Thomas Harriot, Renaissance scientist*, pp. 129–65.

[3] John Henry, 'Thomas Harriot and atomism: a reappraisal', *History of science*, 20 (1982), 267–96.

[4] John W. Shirley, 'An early experimental determination of Snell's Law', *American journal of physics*, 19 (1951), 507–8; and idem, *Harriot. A biography*, pp. 380–88. On Descartes's discovery of the law of refraction see, John A. Schuster, 'Descartes *opticien*: the construction of the law of refraction and the manufacture of its physical rationales, 1618-29', in Stephen Gaukroger, John Schuster and John Sutton (eds), *Descartes' natural philosophy* (London and New York, 2000), pp. 258–312.

[5] So Charles Cavendish is supposed to have claimed to Roberval, as reported by John Wallis. See, Jacqueline A. Stedall, *The Greate Invention of Algebra: Thomas Harriot's*

Treatise on Equations (Oxford, 2003), pp. 28–9. See also Muriel Seltman, 'Harriot's algebra: reputation and reality', in Fox (ed.), *Thomas Harriot, an Elizabethan man of science* (Aldershot, 2000), pp. 153–85.

[6] Indeed, Zach compared Harriot to Kepler as well as to Galileo. See Shirley, *Harriot. A biography*, p. 16. Harriot has also been compared to the Danish astronomer, alchemist, and natural philosopher, Tycho Brahe. See J. A. Lohne, 'Thomas Harriot, 1560–1621: the Tycho Brahe of optics', *Centaurus*, 6 (1959), 113–21.

[7] Dr Schemmel does not need to address the differences which are the focus of this paper, and does not do so. His approach remains entirely valid, however. See the works cited in note 1 above.

[8] Stephen Pumfrey, 'Was Harriot the English Galileo? An answer from patronage studies', *Bulletin of the Society for Renaissance Studies*, 21 (2003), 11–22.

[9] Pumfrey, 'Was Harriot the English Galileo?' *op. cit.* (note 8), and Stephen Pumfrey and Frances Dawbarn, 'Science and patronage in England, 1570–1625', *History of science*, 42 (2004), 137–88. Mario Biagioli, *Galileo, courtier. The practice of science in the culture of Absolutism* (Chicago, 1993), and idem, *Galileo's instruments of credit: telescopes, images, secrecy* (Chicago, 2006).

[10] Details of Harriot's biography can be found, of course, in Shirley, *Harriot. A biography*.

[11] Shirley, *Harriot. A biography*, which can be supplemented by his 'The scientific experiments of Sir Walter Ralegh, the Wizard Earl, and the Three Magi in the Tower, 1603–1617', *Ambix*, 4 (1949–51), 52–66, and 'Sir Walter Raleigh and Thomas Harriot', in Shirley (ed.), *Thomas Harriot, Renaissance scientist*, pp. 16–35. R. H. Kargon, *Atomism in England from Hariot to Newton* (Oxford: Oxford University Press, 1966); Jacquot, 'Harriot, Hill, Warner and the new philosophy', *op. cit.* (note 1).

[12] Pumfrey, 'Was Harriot the English Galileo?', *op. cit.* (note 8), p.16 (quoting from Shirley, *Harriot. A biography*, p. 349).

[13] See Ernest A. Strathmann, 'The History of the World and Ralegh's scepticism', *Huntington Library quarterly*, 3 (1940), 265–87; and Jean Jacquot, 'Thomas Harriot's reputation for impiety', *Notes and records of the Royal Society*, 9 (1952), 164–87.

[14] On Galileo's tactless handling of the concerns of contemporary Catholic theologians see, for example, Ernan McMullin (ed.), *The Church and Galileo* (Indiana, 2005).

[15] Galileo Galilei, Letter to Belisario Vinta, May 1610. For a full discussion of Galileo's negotiations see Biagioli, *Galileo, courtier*, *op. cit.* (note 9).

[16] See Nicholas Jardine, 'Epistemology of the sciences', in Charles B. Schmitt and Quentin Skinner (eds), *Cambridge history of Renaissance philosophy* (Cambridge, 1988), pp. 685–711; Jens Høyrup, 'Philosophy: accident, epiphenomenon, or contributory cause of the changing trends of mathematics—a sketch of the development from the Twelfth through the Sixteenth Century', in idem, *In Measure, number, and weight: studies in mathematics and culture* (Albany, 1994), pp. 123–71; Peter Dear, *Discipline and experience: the mathematical way in the Scientific Revolution* (Chicago, 1995); and Paolo Mancosu, *Philosophy of mathematics and mathematical practice in the Seventeenth Century* (Oxford, 1996).

[17] See, for example, Jardine, 'Epistemology of the sciences', *op. cit.* (note 16), pp. 697–702.

[18] See, for example, William A. Wallace, 'Traditional natural philosophy', in Schmitt and Skinner (eds), *The Cambridge history of Renaissance philosophy*, *op. cit.* (note 16), pp. 201–35; and Steven J. Livesey, 'Science and theology in the fourteenth century: the subalternate sciences in Oxford commentaries on the *Sentences*', *Synthese*, 83 (1990), 273–92; Peter Harrison, 'Physico-theology and the mixed sciences: the role of theology

in early modern natural philosophy', in Peter R. Anstey and John A. Schuster (eds), *The science of nature in the seventeenth century: patterns of change in early modern natural philosophy* (Dordrecht, 2005), pp. 165–83.

[19] For more detailed discussions see Alan Gabbey, 'Newton's *Mathematical Principles of Natural Philosophy:* A Treatise of Mechanics?', in P. Harman and A. Shapiro (eds), *The Investigation of Difficult Things: Essays on Newton and the History of the Exact Sciences* (Cambridge: Cambridge University Press, 1992), pp. 305-22; idem, 'Between *ars* and *philosophia naturalis:* Reflections on the Historiography of Early Modern Mechanics', in J. V. Field and F. A. J. L. James (eds), *Renaissance and Revolution: Humanists, Scholars, Craftsmen and Natural Philosophers in Early Modern Europe* (Cambridge: Cambridge University Press, 1993), pp. 133-45; and Helen Hattab, 'From mechanics to mechanism: the *Quaestiones mechanicae* and Descartes' physics', in Anstey and Schuster (eds), *The science of nature in the seventeenth century: patterns of change in early modern natural philosophy* (Dordrecht, 2005), pp. 99–129.

[20] Paul Lawrence Rose, *The Italian Renaissance of mathematics: studies on Humanists and mathematicians from Petrarch to Galileo* (Geneva, 1975); Mario Biagioli, 'The social status of Italian mathematicians, 1450–1600', *History of science,* 27 (1989), 41–95; W. R. Laird, 'Patronage of mechanics and theories of impact in Sixteenth-Century Italy', in Bruce T. Moran (ed.), *Patronage and institutions: science, technology, and medicine at the European Court, 1500–1750* (Woodbridge, 1991), pp. 51–66; Jim Bennett, 'The mechanical arts', in Katharine Park and Lorraine Daston (eds), *The Cambridge history of modern science. Volume 3: early modern science* (Cambridge, 2006), pp. 673–95.

[21] For a fuller discussion of the prolonged resistance by natural philosophers to what has been called 'the mathematization of the world picture' (see note 57 below), see John

Henry, 'The origins of the experimental method—mathematics or magic?', in Hubertus Busche and Stefan Hessbrüggen (eds), *Proceedings of the First ESEMP Congress* (Hamburg, 2008?), in press.

[22] Presumably circulating after the appearance of Georg Rheticus, *De libris revolutionum… D. Doctoris Nicolai Copernici…, narratio prima* (Basel, 1541). On the background to the publication of *De revolutionibus* and Osiander's role in it, see Alexandre Koyré, *The astronomical revolution* (London, 1973), pp. 34–42; and Bruce Wrightsman, 'Andreas Osiander's contribution to the Copernican achievement', in Robert S. Westman (ed.), *The Copernican achievement* (Los Angeles, 1975), pp. 213-243.

[23] [Andreas Osiander], 'To the Reader concerning the hypotheses of this work', in Nicholas Copernicus, *On the revolutions*, translated by Edward Rosen (Baltimore, 1992), p. XX.

[24] Robert S. Westman, 'The astronomers' role in the Sixteenth Century: a preliminary survey', *History of science,* 18 (1980), 105–47, quotation pp. 108–9.

[25] Copernicus, 'To His Holiness, Pope Paul III', *On the revolutions*, *op. cit.* (note 23), p. 5 (although Rosen translates this famous phrase, 'Mathemata mathematicis scribuntur', as 'Astronomy is written for astronomers').

[26] See Koyré, *The astronomical revolution*, *op. cit.* (note 22), pp. 43–54.

[27] Johannes Kepler, *Astronomia nova ΑΙΤΙΟΛΟΓΗΤΟΣ [aitiologētos], seu physica coelestis, tradita commentariis de motibus stellæ Martis, ex observationibus G. V. Tychonis Brahe* (Prague, 1609). This has been translated by William H. Donahue: Johannes Kepler, *New astronomy* (Cambridge, 1992).

[28] Galileo Galilei, *Siderius nuncius, or the sidereal messenger,* [1610] translated by Albert Van Helden (Chicago, 1989); and idem, *Dialogue concerning the two chief*

world systems—Ptolemaic and Copernican, [1632] translated by Stillman Drake (Berkeley, 1953). The argument for the Earth's motion based on the tides appears in the *Dialogue* on the fourth day.

[29] William R. Shea, 'Scheiner, and the interpretation of sunspots', *Isis*, 61 (1970), 498–519; idem, *Galileo's Intellectual Revolution* (Basingstoke, 1972), pp. 49–74; and Jean Dietz Moss, *Novelties in the Heavens: Rhetoric and Science in the Copernican Controversy* (Chicago, 1993), pp. 97–125. North, 'Thomas Harriot and the first telescopic observations of sunspots', *op. cit.* (note 2); and Clucas, 'Harriot and the Field of Knowledge', in Fox (ed.), *Harriot, an Elizabethan man of science*, op. cit. (note 5), pp. 93–136, especially pp. 120–22.

[30] Galileo Galilei, *Siderius* nuncius; and idem, *Istoria e dimostrazioni intorno alle macchie solari e loro accidenti...* (Rome, 1613).

[31] Westman, 'Astronomers' role', *op. cit.* (note 24), pp. 108–9.

[32] Robert Norman, *The newe attractive, containing a short discourse of the magnes or lodestone* (London, 1581), p. 9.

[33] For a fuller discussion of this aspect of Norman's work see Henry, 'The origins of the experimental method', *op. cit.* (note 20).

[34] For a fuller account, see John Henry, 'Animism and empiricism: Copernican physics and the origins of William Gilbert's experimental method', *Journal of the history of ideas*, 62 (2001), 99–119.

[35] Henry, 'Animism and empiricism', *op. cit.* (note 34).

[36] Shirley, *Harriot. A biography*, p. 272; North, 'Thomas Harriot and the first telescopic observations of sunspots', p. 147; Hilary Gatti, 'The natural philosophy of Thomas Harriot', in Fox (ed.), *Harriot, an Elizabethan man of science*, pp. 64–93, p. 92.

[37] Jim Bennett, 'Instruments, mathematics, and natural knowledge: Thomas Harriot's place on the map of learning', in Fox (ed.), *Harriot, an Elizabethan man of science*, *op. cit.* (note 29), pp. 137–52, p. 139. See also, Stephen Clucas, 'Thomas Harriot and the field of knowledge in the English Renaissance', *op. cit.* (note 29).

[38] Jacquot, 'Harriot, Hill, Warner…', *op. cit.* (note 1), p. 107.

[39] Letter to Harriot, February 6, 1610; quoted from Shirley, *Harriot. A biography*, p. 400.

[40] Shirley, *Harriot. A biography*, pp. 403–4.

[41] Tycho Brahe, *De nova et nullius aevi memoria prius visa stella* ('On the new and never previously seen star', Copenhagen, 1573).

[42] Tycho Brahe, *De mundi aetherei recentioribus phaenomenis* ('Concerning the new phenomena in the ethereal world', Uraniburg, 1588).

[43] Wallace, 'Traditional natural philosophy', *op. cit.* (note 18), pp. 216–7; and Volker, R. Remmert, '"Whether the stars are innumerable for us?": astronomy and Biblical exegesis in the Society of Jesus around 1600', in Kevin Killeen and Peter J. Forshaw (eds), *The word and the world: Biblical exegesis and early modern science* (Basingstoke and New York, 2007), pp. 157–73.

[44] William Gilbert, *De mundo nostro sublunari philosophia nova* ('New philosophy about our sublunary world', Amsterdam, 1651), Ch. XIV, p. 173. Although unpublished at his death in 1603, the *De mundo* may have been written as early as the 1580s. See Sister Suzanne Kelly OSB, *The 'De mundo' of William Gilbert* (Amsterdam, 1965).

[45] J. W. Shirley, 'Thomas Harriot's lunar observations', in E. Hilfstein, Paweł Czartoryski, Frank D. Grande (eds), *Science and history: studies in honor of Edward Rosen* (Wrocław, 1978), pp. 283–308; and Amir Alexander, 'Lunar maps and coastal outlines: Thomas Harriot's mapping of the Moon', *Studies in history and philosophy of*

science, 29 (1998), 345–68. For a general survey see E. A. Whitaker, 'Selenography in the seventeenth century', in R. Taton and C. Wilson (eds), *Planetary astronomy from the Renaissance to the rise of astrophysics. Part A: Tycho Brahe to Newton* (Cambridge, 1989), pp. 119–43.

46 William R. Shea, 'Scheiner, and the interpretation of sunspots', *op. cit.* (note 29); idem, *Galileo's Intellectual Revolution*, *op. cit.* (note 29), pp. 49–74; and Moss, *Novelties in the Heavens*, *op. cit.* (note 29), pp. 97–125.

47 For a full account of Descartes' life and work see Stephen Gaukroger, *Descartes: an Intellectual Biography* (Oxford, 1995). See also Jean-Robert Armogathe, 'The rainbow: a privileged epistemological model', in Gaukroger, Schuster and Sutton (eds), *Descartes' natural philosophy*, *op. cit.* (note 4), pp. 249–57.

48 Galileo Galilei, *Discorsi e dimostrazioni matematiche: intorno à due nuoue scienze, attenenti alla mecanica & i mouimenti locali* (Leiden, 1638).

49 Cornelis de Waard (ed.), *Journal tenu par Isaac Beeckman de 1604 à 1634*, 4 vols (The Hague, 1939–1953); and Klaas van Berkel, *Isaac Beeckman (188–1637) en de mechanisering ven het wereldbeeld* (Amsterdam. 1983).

50 Jon V. Pepper, 'Harriot's manuscript on the theory of impacts', *Annals of science*, 33 (1976), 131–51, p. 133. See also the works by Matthias Schemmel cited in note 1 above, and Stephen Clucas, '"No small force": natural philosophy and mathematics in Thomas Gresham's London', in Francis Ames-Lewis, *Sir Thomas Gresham and Gresham College: studies in the intellectual history of London in the Sixteenth and Seventeenth Centuries* (Aldershot: Ashgate, 1999); and for a more general treatment of the topic, Friedrich Steinle, 'From principles to regularities: tracing "laws of nature" in early modern France and England', in Lorraine Daston and Michael Stolleis (eds), *Natural*

law and laws of nature in early modern Europe: jurisprudence, theology, moral and natural philosophy (Aldershot, 2008).

[51] Nathaniel Torporley, 'A synopsis of the controversie of atoms', British Library, Birch MS. 4458, f. 6 (copied f. 7–8). This is reprinted in Jacquot, 'Harriot's reputation', *op. cit.* (note 13), pp. 183–6. Harriot's own discussion of atomistic ideas is chiefly confined to the pages entitled *De infinitis*, and these are predominantly concerned with mathematical rather than philosophical issues. Copies of Harriot's *De infinitis* can be found in the British Library, Add. MS 6782b, f.362r–374v; Add. MS 6785b, f. 436–7; and Harley MS 6002, f. 2–10. For a fuller discussion of these papers see Henry, 'Harriot and atomism', *op. cit.* (note 3).

[52] Letter to Kepler, July 13, 1608, reprinted as letter no. 497, in Johannes Kepler, *Gesammelte Werke*, edited by Max Caspar *et al.*, 21 vols (Munich, 1938-2002), vol. 16, pp. 172–73.

[53] Henry, 'Harriot and atomism', *op. cit.* (note 3), pp. 286–7.

[54] Henry, 'Harriot and atomism', *op. cit.* (note 3). On Newton's version of atomism see, for example, R. S. Westfall, 'Newton and the Hermetic tradition', in Allen G. Debus (ed.), *Science, medicine and society in the Renaissance*, 2 vols (New York, 1972), ii, 183-98; and idem, 'Newton and Alchemy', in Brian Vickers (ed.), *Occult and scientific mentalities in the Renaissance* (Cambridge, 1984), pp. 315-35.

[55] On Galileo's use of atomistic arguments see A. Mark Smith, 'Galileo's theory of indivisibles: revolution or compromise?' *Journal of the history of ideas*, 37 (1976), 571-88; and Henry, 'Harriot and atomism', *op. cit.* (note 3). Aristotle denies that a collection of indivisibles can make up a magnitude in *Physics*, Book 6, Chapter 1.

[56] On Descartes, see Gaukroger, *Descartes: an intellectual biography*, *op. cit.* (note 47).

[57] Literature on Descartes is vast, but as good a starting point as any is Gaukroger, *Descartes: an intellectual biography*, *op. cit.* (note 47). For a specific example of the innovatory nature of Descartes's work see John Henry, 'Metaphysics and the origins of modern science: Descartes and the importance of laws of nature', *Early science and medicine*, 9 (2004), 73–114.

[58] On Descartes's theory of matter, see William B. Ashworth, 'Christianity and the mechanistic universe', in David C. Lindberg and Ronald L. Numbers (eds), *When science and Christianity meet* (Chicago, 2003), pp. 61–84, especially pp. 62–5; and Peter Dear, *Revolutionizing the sciences: European knowledge and its ambitions, 1500–1700* (Basingstoke, 2001), pp. 80–100.

[59] The 'mathematization of the world-picture' was claimed as characteristic of the Scientific Revolution by E. J. Dijksterhuis, Alexandre Koyré, and E. A. Burtt, and was an insight that proved highly influential. See H. Floris Cohen, *The Scientific Revolution: a historiographical inquiry* (Chicago, 1994), pp. 59–97. Jim Bennett, 'The mechanics' philosophy and the mechanical philosophy', *History of science,* 24 (1986), 1–28; idem, 'The challenge of practical mathematics', in S. Pumfrey, P. Rossi and M. Slawinski (eds), *Science, culture and popular belief in Renaissance Europe* (Manchester, 1991), pp. 176–90; and idem, 'The mechanical arts', *op. cit.* (note 20).

[60] James A. Bennett, 'Mechanics' philosophy', op. cit. (note 59), p. 24.

[61] The classic account of the 'mathematization of the world picture' by Alexandre Koyré and others (see note 57 above) sees mathematics being introduced into a previously *qualitative* physics, but the latest historiography presents a story in which causal physical explanations are introduced into mathematical analyses of physical phenomena.

[62] See Antonio Favaro, 'Introduction', in Galileo Galilei, *Dialogues concerning two new sciences*, translated by Henry Crew and Alfonso de Salvio (New York, 1914), p. xii. For further discussion of negative attitudes towards mathematics see Henry, 'Mathematics or magic?' *op. cit.* (note 21); and idem, '"Mathematics made no contribution to the public weal": why Jean Fernel became a physician', in Lesley Cormack (ed.), *Mathematical practitioners and the Scientific Revolution* (Chicago, 2008?), in press.

[63] Isaac Newton, *The Principia: mathematical principles of natural philosophy*, translated by I. Bernard Cohen and Anne Whitman (Berkeley, 1999), p. 943.

[64] Indeed, Osiander's defence of Copernicus, in his interpolated prefatory note to *De revolutionibus*, was to point out that he had not offered any explanation of the movement of the Earth, and therefore had not transgressed the boundary between mathematics and physics. See above, and Westman, 'Astronomers' role', op. cit. (note 24).

[65] Harriot can be compared here to Francis Bacon. Francis Bacon left his readers in little doubt that natural philosophy had so far led only to dead ends, but, not being a mathematician, he called for a reform of natural philosophy. The separation of mathematics from natural philosophy at this time is again underscored by the fact that it never occurred to Bacon to use mathematics in his reform of natural knowledge. See Henry, 'Mathematics or magic?' *op. cit.* (note 21).

[66] As suggested, remember, by Westman, 'Astronomers' role', *op. cit.* (note 24), pp. 108–9.

[67] See Paolo Palmieri, 'Re-examining Galileo's theory of tides', *Archive for the History of the Exact Sciences*, 53 (1998), 223–375. Palmieri succeeds in showing that Galileo had a powerful intuition about the oscillatory nature of the tides, but the fact remains

that Galileo saw these oscillations as the result solely of the motions of the Earth, whereas, as Laplace showed (see Palmieri, p. 356), following Newton, they are set up by the gravitational attractions of the Moon and the Sun.

[68] Galileo 'demonstrates' that the planets, once set in perfectly circular motions about the Sun would continue to move this way indefinitely without either speeding up or slowing down, in the 'Second Day' of the *Dialogue concerning the two chief world systems*.

[69] Newton, *The Principia*, 'General Scholium', *op. cit.* (note 63), p. 939. For an excellent general discussion see Alan Gabbey, 'The mechanical philosophy and its problems: mechanical explanations, impenetrability, and perpetual motion', in J. C. Pitt (ed.), *Change and progress in modern science* (Dordrecht, 1985), pp. 9–84.

[70] Much work remains to be done on Walter Warner but for now, consider Jacquot, 'Harriot, Hill, Warner…', *op. cit.* (note), pp. 117–25; Henry, 'Occult qualities and the experimental philosophy: active principles in pre-Newtonian matter theory', *History of science,* 24 (1986), 335-81, pp. 340–42; and Jan Prins, *Walter Warner (ca. 1557–1643) and his notes on animal organisms* (Utrecht, 1992); Stephen Clucas, 'The atomism of the Cavendish Circle: a reappraisal', *The seventeenth century*, 9 (1994) pp.247-273; and idem, 'Corpuscular matter theory in the Northumberland Circle', in Christoph Lüthy, John Murdoch, and William Newman (eds), *Late Medieval and Early Modern Corpuscular Matter Theory* (Leiden, 2001), pp. 181-207.

[71] The sixteenth century is generally recognised as a period of great achievement in the history of mathematics, but this period is often overlooked in histories of philosophy. The standard account of developments in natural philosophy at this time focus on what are generally seen as fanciful attempts at system-building by the Italian Renaissance 'nature philosophers' (Telesio, Patrizi, Bruno, and Campanella), and the false lights of

Paracelsianism and Rosicrucianism. See Brian P. Copenhaver and Charles B. Schmitt, *Renaissance philosophy* (Oxford, 1992). Literature on Bacon's would-be reforms in natural philosophy is vast but see, for example, Stephen Gaukroger, *Francis Bacon and the transformation of early-modern philosophy* (Cambridge, 2001).

[72] For the full transcription, and a translation from the Latin, see Shirley, *Harriot. A biography*, p. 474.

[73] Pumfrey, 'Was Harriot the English Galileo?' *op. cit.* (note 8), p. 18. On Warner, see the articles cited in note 68 above and Shirley, *Harriot. A biography*.

[74] With the possible exception, as said earlier, of a brief and inconsequential flirtation with atomism.